be represented by lifting the chain with a weak thread, and by having the end of the chain fastened to a pretty strong spring. When the thread broke the spring would pull the chain back quickly, would pass its position of equilibrium, and thus commence a series of rapid vibrations on each side of this position; the vibrations would gradually die away owing to the energy of the spring being gradually spent, partly on friction in itself, and partly in sending waves along the chain. In actually performing the experiment, an india-rubber tube or limp thin rope is better than a chain when hung horizontally, as the chain is so heavy; when it can be hung vertically, a chain does very well. In the description it simplifies matters to describe a chain, because it is easier to talk of a link than of a bit of the rope: a link has an individuality that identifies it, while a bit of the rope is so indefinite that it is not so easy to keep in mind any particular bit. Consider now what these waves are, what sort of motion originates them. When the spring first starts, the near parts of the chain move first. What happens to any link? One end of it moves down before the other. What sort of motion, then, has the link? It must be rotating. Thus it is that change in the displacement is generally accompanied by rotation of the links. Thus it is that change in electric strain is accompanied by magnetic strain. The analogy goes farther than this. Each wave thrown off may be described as a wave of displaced or as a wave of rotating links, and the most displaced are at any time the most rapidly rotating links. Just in the same way, what have hitherto been called waves of electric force may also be looked upon as waves of magnetic force. Because there are two aspects in which the motion of the chain may be viewed does not diminish from the essential unity of character of the wave-motion in its waves; and similarly the fact that these Hertzian waves have an electric and a magnetic aspect does not diminish from the essential unity of character of the wave-motion in them. At the same time the two elements, the displacement of a link and the rotation of a link, are quite distinct things; either might exist without the other; it is only in wave propagation that they essentially co-exist. In the same way electric strain and magnetic strain are quite different things; though in wave-motion, and indeed whenever energy is transmitted from one place to another by means of the ether, they essentially co-exist.

FIVE YEARS' PULSE CURVES.

OVER five years ago it occurred to me that there would be considerable interest in keeping a systematic record for some time of the rate of pulsation, *i.e.* of the number of beats (per minute) of the pulse. I therefore commenced the practice by taking, every night, an observation of my own pulse; these observations, originally undertaken solely for my own personal interest, have been continued without intermission up to the present time; and, on throwing the results into a graphic form, I found so close a symmetry and concord between the curves for these five years, that I thought it might be interesting to readers of NATURE to have these results put before them.

First, then, as to the method adopted in these observations. I count the pulse beats for one minute[1] every night[2] before retiring to bed, and invariably while in a standing posture. From the records thus obtained the average for each month is deduced in the usual way, viz. by adding together all the numbers for the month, and dividing by the number of days on which observations were taken. With regard to this important consideration—*i.e.* of the number of observations, since an insufficiency thereof would, of course, greatly vitiate the value of my curves—I may state that during the first four years I omitted to take an observation on only seventeen nights altogether. During the fifth year, I find twenty-one observations missed. Nevertheless, the *net* data from which the curves are deduced are not quite so abundant as this statement would imply; for, in calculating the monthly average, I have invariably struck out altogether *all readings above* 79. My reason for this procedure was simply that I wished to obtain a curve showing the normal pulsations; now, anything much above 75 is abnormally high (especially in my own individual instance, for it will be noticed that my pulse is below the usual average of 70), and I can nearly always assign a distinct cause, such as the feverishness caused by a cold, or excitement, or recent exercise; it therefore appeared to me fairest to knock out altogether the results of such disturbing causes, and since for this purpose an arbitrary line must be drawn somewhere, I decided to draw it at 79. On the other hand, however, I have retained all the other readings, no matter how low they might be, although the "fifties" are very common, and occasionally even the "forties" have been touched. It might, perhaps, be thought that these very low readings should be neglected equally with the very high, but such a course appeared to me altogether illegimate, both because such low readings seemed, judging from their occurrence, to be, so to speak, *normally* caused, and unassignable to any distinct extraordinary cause known to myself, and also because I should hardly have known where to draw a minimum line. However, I now regret that the readings below 50, at any rate, were not rejected; but such readings are so extremely rare that they cannot have much influenced the curves. In order that the reader may judge for himself on what data these curves are founded, I have appended a table showing the *net* number of readings from which each monthly average was drawn, and have also stated (in brackets) the number of readings below 60 included in each month.

Turning now to the curves themselves, this monthly average is shown by the *thin* line. It is necessary to explain that these curves were drawn by marking the monthly average by a dot on the *extreme right* of each space representing a month. I was undecided for some time whether to adopt this plan or to mark this dot in the *middle* of each monthly space; but after trying both plans I concluded that now adopted to be the simpler. The actual curves were, of course, obtained by connecting all these dots by straight lines.

On examining this monthly curve, it is at once obvious that there is a strong similarity between the five years; clearly every year the curve falls through the spring, until about midsummer, and then rises wonderfully steadily and regularly in every case (except in 1889) through the autumn to November or December. On the whole, two maxima seem to be indicated—namely, one in November, followed by a fall, and then by a rise to another maximum in February or January. But it will be noticed that in the winter 1889-90 there is the unusual phenomenon of a fall through November, and then the two maxima are replaced by an intermediate maximum reached in December. So that here, in spite of the broad concord and regularity, there was rather too much local irregularity to be altogether satisfactory. In the lower portions of the curves, again, there is even more irregularity. Those of 1887 and 1888 (but emphatically the former) are indeed remarkably free from aberration; but in 1886 there is an extraordinarily abrupt and irregular rise through July, followed by a compensating fall through August. In 1890 there is an almost identical irregularity in the same two months; while in 1889 we have a remarkable irregularity in the spring. Now these irregularities puzzled me a good deal; still, in each case

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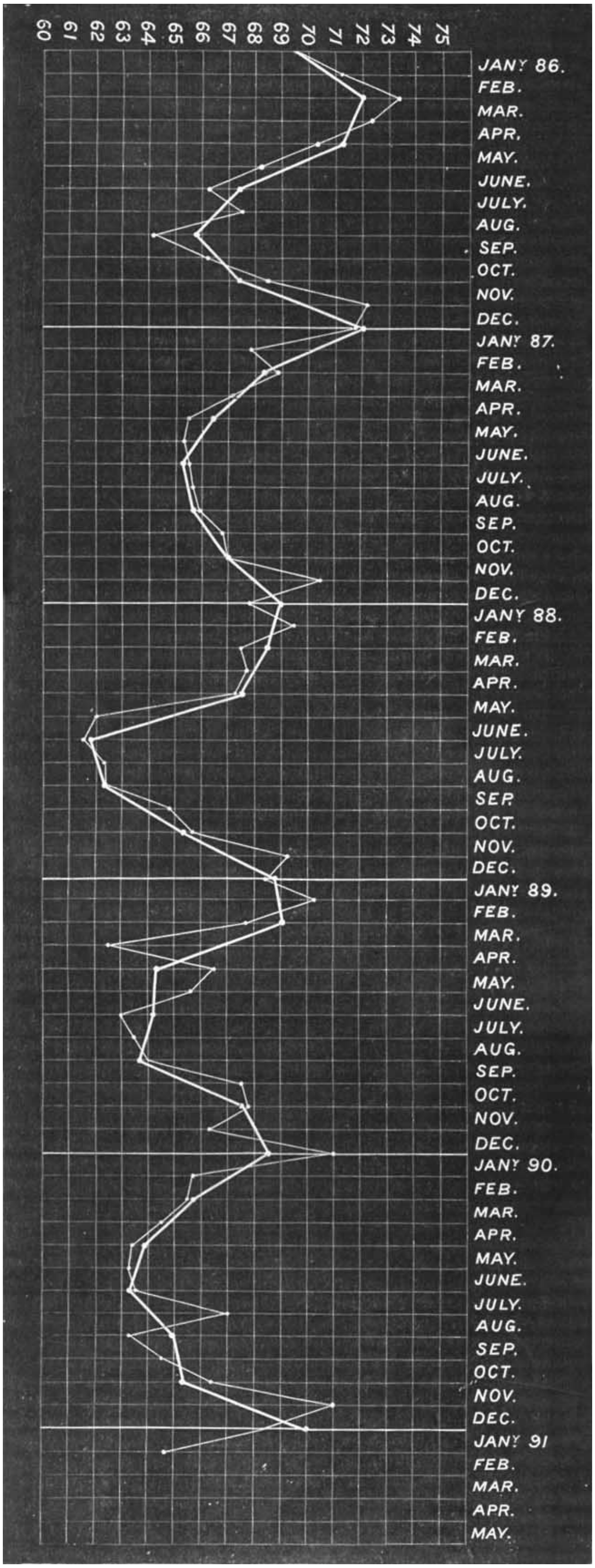
[1] Thus avoiding the considerable error that is introduced by counting for, say, fifteen seconds only, and multiplying.

[2] The time has varied from 23.0 o'clock to 1 o'clock.



(except that of the winter irregularity, 1889–90), I could assign a fairly plausible explanation. For instance, during the summer of 1886 I was under medical treatment: in July of 1890 I was touring among the Swiss mountains: while at the end of February 1887 I had removed from a low-lying northern suburb, to a rather higher southern one; this change might with some plausibility be considered as the possible disturbing cause in the 1889 spring curve.

Nevertheless, looking at the results as a whole, I was not satisfied with the curves: it appeared to me as by no means improbable that the monthly average was calculated on a rather too short period, thus allowing temporary disturbing causes to manifest themselves unduly. I therefore determined to try the effect of calculating the averages on a *two-monthly* period, throwing into one total January and February, March and April, May and June, July and August, September and October, November and December respectively. On drawing the curves corresponding to these averages (*thick*-lined curve), I was delighted to find order and symmetry completely regnant: all the aberrations have of course disappeared, and order is supreme. This two-monthly curve clearly shows a single maximum in winter, followed by a fall to the minimum at midsummer, and then by a rise to the winter maximum.

It is evident that the curves for all five years are very closely similar, though by no means identical[1] in nature; but I am especially anxious to point out the extraordinary symmetry displayed by the *curves on either side of a maximum or minimum* point. For instance, the curves for the following periods,

1886 July–October,
,, April–December,
,, November–February 1887,
1888 November–May 1889,
1889 The whole year,

are wonderfully symmetrical, in some cases even being almost geometrically exact.

What, however, may be the exact interpretation of these curves I must leave it to those better acquainted than myself with physiology to decide; but it is worth noting that these curves are exactly contrary to the statement in Michael Foster's text-book, that the pulse *is said* to rise in summer.[2]

The following is the table above referred to as showing the *net* data for each month, and also (in brackets) the number of readings below 60 included in each case:—

1886.		1888.	
January	28	January	25 (1)
February	19	February	23 (1)
March	21	March	24 (3)
April	25	April	30 (4)
May	30 (1)	May	30 (11)
June	29	June	30 (11)
July	30 (1)	July	31 (10)
August	28 (8)	August	28 (9)
September	29 (2)	September	28 (5)
October	29 (1)	October	31 (5)
November	25	November	26 (2)
December	23	December	28 (3)
1887.		1889.	
January	28 (2)	January	25
February	26 (1)	February	23 (2)
March	30 (3)	March	31 (9)
April	27 (4)	April	28 (8)
May	27 (3)	May	28 (3)
June	25 (5)	June	29 (10)
July	31 (3)	July	28 (4)
August	30 (6)	August	28 (7)
September	28 (2)	September	27 (4)
October	30 (2)	October	30 (2)
November	23	November	26 (4)
December	25 (2)	December	25 (1)

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[1] Seeing how very many causes must co-operate in producing the one resultant—of pulsation-rate—it would be very strange if the curves for different years *were* identical.

[2] I quote from the third edition.

1890.

January	27 (5))	July	24 (2)
February	24 (5)	August	30 (8)
March	28 (4)	September	29 (7)
April	28 (7)	October	24 (5)
May	27 (7)	November	22 (1)
June	26 (8)	December	30

If these numbers be compared with the curves, it will be found that in a rough way they agree with them; the diminishing number of these low readings every autumn, no less than their increase towards the summer, being obviously correlated with the rise and fall of the curves.

F. H. Perry Coste.

THE SCIENCE MUSEUM AND GALLERY OF BRITISH ART AT SOUTH KENSINGTON.

VIGOROUS protests continue to be made against the appropriation, for the new Gallery of British Art, of the site which ought to be used, as originally intended, for the Science Museum. Several letters on the subject by men of high authority have been printed in the *Times;* and on Tuesday a deputation, which could not but command attention and respect, waited upon Lord Cranbrook and Mr. Goschen to represent to them the opinions held by all who are in a position to form a trustworthy judgment on the question. The Government are still engaged in considering the matter, and it is to be hoped that they are receiving and giving heed to the counsel of their natural advisers, although, unfortunately, this is *a priori* extremely doubtful.

We print the letters addressed to the *Times* by Sir F. Bramwell, Mr. Poynter, and Sir J. Coode, and an account of the proceedings of the deputation on Tuesday.

It has for many years been recognized that the science collections at South Kensington are housed in a manner which largely diminishes their value for their principal use—viz. that in connection with the Royal Normal School of Science.

This school, as every one knows, is, as regards its main building, situated on the east side of Exhibition Road, while the collections are scattered about in the South Gallery and in the West Gallery adjacent to Queen's Gate.

In 1885 the Government appointed an inter-departmental committee to consider the subject and to report, and they nominated me, as being unconnected with any department, chairman of the committee. The committee (with one dissentient) reported in the sense that on the land lying west of Exhibition Road, and between that road and Queen's Gate, suitable buildings should be erected according to a complete design, but that they should be carried out in successive portions. Nothing was done on this report.

In 1889 another committee was appointed; this committee made very similar recommendations, and last year the Government acquired further land.

There are now on the west side of Exhibition Road, and immediately opposite the science schools, the observatories used by Mr. Norman Lockyer, and also a newly-erected physical laboratory.

Everything seemed to be, after all these years of waiting, in train for affording the needed accommodation, when, incredible as it must appear, the Chancellor of the Exchequer announced that the whole of this well-considered and satisfactory arrangement is to be given up. He stated it had been determined to sweep away the observatories and the physical laboratory, already on the west side of the road, and close to the science schools, and to devote this particular plot of ground to a picture gallery. I look upon this as a most disastrous proceeding, and one that, in the interest of the great National Department at South Kensington, should not be entertained for one moment.

Any one who will take the pains to visit the ground, or even to look at an accurate plan of it, will see that there is plenty of good space available for the picture gallery without interfering with the needs of the science collection, and that the notion of building it where proposed is so thoroughly preposterous that, as our American friends say, it must have originated in "pure cussedness."

Frederick Bramwell.

No. 5 Great George Street, Westminster, May 9.

Sir Frederick Bramwell in his letter of this morning points out the disastrous effect on the interests of the national Department of Science at South Kensington which will result from the intrusion of the new Gallery of British Art, to be planted precisely on the spot where it will cause the greatest amount of inconvenience. To an artist a still more flagrant instance of "pure cussedness" in this matter would appear to be that the building should be placed where it can have no connection with the existing galleries, when there is a piece of ground higher up the road in immediate connection with them.

The galleries on the east and west of the Horticultural Gardens, which were built for pictures at the time when there was a scheme for holding annual international exhibitions, are, whether by a happy "fluke" or by careful calculation on the part of their constructor, General Scott, without doubt the best lighted and the best proportioned picture galleries that have ever been constructed in England. Sir Frederick Leighton has, I know, expressed this opinion, and every artist who exhibited in these galleries during the three or four years that the exhibitions were held there will, I believe, agree in it: "We never saw our pictures look so well." These galleries are even now being connected by a building crossing the intervening space, the lower half of which will belong to the Imperial Institute, while the upper part is to be available for purposes of exhibition, thus making a connected group, and what would appear to be an unrivalled building for the purposes of a Gallery of British Art.

Why these buildings, acknowledged to be as good as they can be, and actually ready on the spot, should not be used for this purpose, according to what I understand was the original and nearly accepted scheme, it is somewhat difficult to understand. If the building for which £80,000 has been so liberally offered were placed higher up the road, above the Technical Institute, where there is a piece of ground available, it would back immediately on the Eastern Gallery, in which the Indian collection is now housed, thus affording provision for the extension of the collection, which is growing annually by the addition of the pictures purchased under the Chantrey bequest, and to which it is certain that further considerable additions will constantly be made by gift and bequest as soon as there is a place in which they can be properly and permanently exhibited.

Also, there is for once, if advantage be taken of it, an opportunity for carrying out a reasonable and consistent scheme for both science and art.

Edward J. Poynter, R.A.

28 Albert Gate, S.W., May 11.

Having served on the Committee on Machinery and Inventions in connection with the Science and Art Department of the Committee of Council on Education, I desire most emphatically to endorse the protest of Sir Frederick Bramwell which appears in your columns of this day's date.

Although the fees received from patentees up to the end of 1885 exceeded the expenditure of the Patent Office by upwards of 2½ millions sterling, nothing practically has been done to put the Patent Museum and Museum of Machinery and Inventions in an efficient condition.

Year after year the Committee, of which I am a member, has urged that more space should be given to the authorities at South Kensington, and now, when it was thought the recommendations were about to be realized, it is asserted that the promised site is to be devoted to a picture gallery.

I sincerely trust that this intention may not be carried out, but that the site in question, which exactly faces the Royal College of Science, will be appropriated for the science collections, to which purpose it has long been assigned.

Jno. Coode, President.

The Institution of Civil Engineers, 25 Great George Street,
Westminster, May 11.

The deputation which waited upon Lord Cranbrook, the Lord President of the Council, and Mr. Goschen was large and representative. Mr. Plunket, M.P., First Commissioner of Works, was also present. Among the deputation were: Sir William Thomson (President of the Royal Society), Sir Bernhard Samuelson, M.P., Sir George Gabriel Stokes (Past President of the Royal Society), Mr. C. Acland, M.P., Sir Frederick Bramwell, F.R.S., Prof. Story-Maskelyne, M.P., Sir Douglas Galton, C.B., Mr. Poynter, R.A., Prof. Unwin, Mr. Francis